

*SCHEDULES OF REINFORCEMENT  
WITH SKINNER*

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To tell about the pigeon laboratory at Harvard during the period I was there, I must first describe the state of the science at Columbia while I was a graduate student there. The comparison provides a “before and after” which will help me to communicate what happened at Harvard during the years when B. F. Skinner and I worked on *Schedules of Reinforcement*.

The pigeon lab was already operating at Harvard in the fall of 1950 when word reached Columbia that Skinner was looking for someone to assist him. Columbia was the obvious place to look because there was so much activity and excitement there about operant conditioning and a functional analysis of behavior. Keller and Schoenfeld had just completed *Principles of Psychology*, and the introductory course at Columbia was in full swing. We learned of the impact of a laboratory science of behavior on biological science, pressing community problems such as mental illness, education, and rearing children for a better life and a basic understanding of human nature. Everyone had conditioned a rat, read *Walden Two*, and most were impatient for a chance to try out a science of behavior. Some students fantasied a new Institute for Operant Behavior with buildings, equipment and full-time research. Others dreamed of an actual planned community modeled after *Walden Two* where the products of laboratory research could be lived and applied. I’m sure many of today’s laboratories exceed what were then our wildest expectations. For in those days the typical operant experimenter either manually operated switches in a darkened room, or programmed a half dozen relays cannibalized from vending machines. A pressing instrumentation problem was a reliable pellet dispenser, but recording problems were not serious because

only a small amount of behavior was recorded. Experiments, seldom more than an hour long, took place just before the rats were fed.

I was a third-year graduate student when, hearing of the chance to work with Skinner, I made an appointment to go to Cambridge for an interview. I took the midnight train to Boston and wandered around Harvard Square nervously from six in the morning until what I thought would be a respectable hour to appear at Skinner’s office. The interview was easy once I got there. We had a coke, he showed me some of the equipment in the lab, and I was scarcely aware of at what point I knew that I was to come to work in February. Within two hours I was on my way back to New York and Skinner was back in his office writing.

I had finished almost all of my course work at Columbia and was doing exploratory experiments on chaining. A retractable lever came into the cage when the rat pulled a chain suspended from the ceiling. These were called exploratory experiments because they preceded the real experiment; because only one or two animals were used; because the procedures as well as the apparatus were constantly adjusted during the experiments; and because it was impossible to know in advance what was going to happen. Experiments in which an animal served as its own control were not quite acceptable at Columbia as yet.

Because Skinner wanted me to be in Cambridge by the first of February, completing a Ph.D. dissertation before I left posed a large problem. At Columbia, getting a thesis topic approved was quite an involved process. First there were informal tests with faculty and student. These consisted of discussions in the corridor with other graduate students and visits to several professors’ offices. The proposed experiment was received very differently in different places. First, there were the kind ear and probing questions of Professor

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*From: Dews, P. B. (Ed.). (1970). Festschrift for B. F. Skinner (pp. 37–46). New York: Irvington. Reprinted with the kind permission of Irvington Publishers, New York.*

Keller who listened gently until there was no more time. Later in the day of my "test" with Keller, I found myself redoing the plan as I tried to explain answers to his questions. Others were not so gentle. A thesis plan also had to pass muster of a formal departmental meeting. Since I had not even gotten by the informal test when I returned from my interview with Skinner, it was clear that the usual process was much too long and labored to meet Skinner's deadline, so chaining was put aside, for the time being. Instead I formulated a hypothesis, built equipment, ordered fifty genetically controlled Wistar rats, and tested the hypothesis that a stimulus present during conditioning would influence the number of performances the rat would emit when reinforcement was discontinued.

The laboratory was in operation when I arrived in Cambridge. Several graduate students were preparing pigeon demonstrations for Skinner's introductory course and there were several pigeon boxes with relay control apparatus. The newest behavioral discovery was aperiodic or random-reinforcement (variable-interval) schedules which were programmed by a metal phonograph recording disc covered with plastic. A slow motor turned the disc. A wiper, operating on the outside groove of the disc, like the recording arm of a phonograph, picked up an electric pulse whenever the covering was scraped away. The distribution of scratches around the periphery of the disc made the variable schedule, and the number of scratches determined the average interval of reinforcement.

When I reported to Skinner on my first day, he showed me parts and plans for a variable-ratio programmer for Elinor Maccoby, then completing her Ph.D. in the Social Relations Department. She needed the equipment for an experimental thesis with pigeons, which extended the experiments on random-interval to random-ratio schedules as they were then called. The programmer, already designed by Skinner, was to be built from a stepping switch much along the principle of the motor-driven disc used to arrange a variable-interval schedule.

My first months in the pigeon lab were a strange contrast of days adjusting equipment and experimental procedures for one or two pigeons, and nights at the calculating machine trying log and trigonometric transfor-

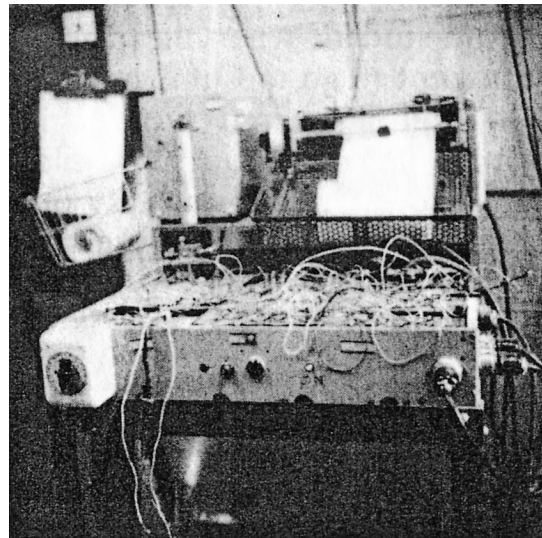


Fig. 1. The relay programming and recording equipment for the chained VI FI equipment. There were three parallel pairs of bars on the tables into which the relay and other control panels were fastened. The pigeon box rested on the shelf below the table and the recorder sat on a bridge across it.

mations of the thesis data to make an analysis of covariance possible. Fortunately, the increase of the frequency of rewards of the former activity could be paced with the early completion of the latter.

While I was building the variable-ratio programmer, I spent the rest of the time during my first week in the lab exploring all of the parts in the drawers and cabinets, reorganizing them according to my own habits, and labeling them to my custom. I found a large store of small electrical and mechanical parts: springs, phosphor bronze, string, glue, bakelite, plexiglas, surplus relays, assortments of capacitors and resistors, cable clamps, lacing cord, soldering supplies, bits and pieces of rubber and plastic, a large box of accumulated nuts and bolts, small odd pieces of metal, wire and cable, surplus electronic relays and electrical devices that could be disassembled for parts, cardboard and paper, motors and a host of the miscellany that seems to come in handy at odd times for unexpected uses. Gerbrands' machine shop down the hall had seemingly endless drawers of bolts and nuts, cotter pins, hex nuts, brass nuts, steel nuts, lock washers, Allen nuts, Phillips head, round-head, flat-head, oval-head, and spline-head screws, wood, machine and metal

screws, brass and steel washers and lock washers. All were stocked in every length, diameter and thread. Further down the hall were the psycho-acoustic laboratory shops, directed by Rufus Grason, where resistors, capacitors and all of the rest were found in the same rows of cabinets and in the same profusion of varying wattages, resistances and capacitance values, accuracy levels and shapes. The pigeon laboratory already had a room dedicated as a shop with a drill press, two long work benches and the usual assortment of hand tools.

Instrumentation was easy and natural, and all components for innovative apparatus construction were immediately at hand. Herbach and Rademan sold surplus electrical equipment by mail order catalogue, even then, and it was the tradition then as it is now to scan the catalogue each month to buy parts and devices that "might be useful sometime." One of the first steps for solution of an instrumentation problem was always to look through the drawers and cabinets to see what suggested itself.

The physical arrangements of the laboratory, the supplies, the equipment and the shop were important factors in determining the kind of research that went on. There were sufficient parts immediately on hand for construction to begin the moment an experiment required new instrumentation. Skinner usually built the first model from what was on hand, seldom waiting because parts needed to be ordered. The prototype was usually makeshift and not quite reliable enough, but it served long enough to prove itself. By then there had been time enough to order proper parts and to build a well-constructed model.

Probably the most serious and pressing instrumentation problem we faced was the design of a reliable cumulative recorder, and the construction of enough of them to service the large number of experiments that ran concurrently. Even more recorders were needed because we developed the habit of using several at once on a single experiment, as in a multiple schedule, to treat the data during recording rather than by numerical manipulations later. The first model used a Ledex rotary switch to drive the pen on the performance scale. By this time the paper drive worked well, using a typewriter platen, with its associated mechanism for holding the

paper, and the Leeds-Northrup glass reservoir pen solved the problem of providing a reliable ink line. Twelve recorders were hardly completed, however, when the experimental sessions lengthened because we learned how to sustain high rates of performance with our pigeons for ten-hour sessions or more. Experiments which recorded two or three thousand pecks at the start of our research soon required 100,000 or more pecks to be recorded during a single experimental session. For a long while, I spent much of my time replacing and repairing rotary solenoids which lasted only a few hundred thousand operations. The discovery of the Automatic Electric stepping switch mechanism, which stood up to the billions of pecks which were recorded on each instrument, freed much time and energy for other purposes.

It was an enormous source of support to move into a laboratory which Skinner had already arranged and stocked. A beginner faces so many anxieties and new problems that without this support I doubt that there would have been enough energy both for producing the physical arrangement of shops, supplies and equipment that is so critical in order to be able to do innovative research, and for actually carrying out an experiment. The pigeon lab set the pattern for all of my later laboratories. For example, I always saved and carried with me a large box of nuts, screws, hardware, assorted junk and parts and devices that accrued when the bench top was swept and that "might be useful someday." For almost ten years, I carried around a 244 pole stepping switch (purchased from surplus for a dollar or two) before I finally threw it out.

During my first months with Skinner and the pigeon lab, I learned a great deal about how to run a laboratory, design and interact with experiments, and think through instrumentation and research problems. The teaching process was so natural but subtle, that I had no awareness that I was learning anything new or that the research we were carrying out was a departure from the existing body of knowledge. It was not until months later, around the time we gave our first paper on schedules (Skinner on mixed and I on multiple schedules), that I began to consciously sense that our work was extending and departing from the current literature.

I think that part of the reason for the del-

icacy and smoothness of the learning process was Skinner's natural style of creating the conditions which allowed learning to take place rather than teaching or telling me things. In retrospect, my personal experience in the spring of 1950 contained many examples of how the laboratory environment contained supplementary and collateral variables which supported my behavior so long as they were needed, and which faded out as I developed my own ways of providing the same support. The first task assigned to me in the laboratory, constructing the random-ratio (variable-ratio) programmer for Elinor Maccoby's experiment, served to move me into action at my own pace and with support. The device had already been designed and the components were at hand. Although it was a simple device which I could now complete in an hour or two, I spent two or three days poking away at it, redoing it several times and at the same time getting used to the color of the walls and the other features of my new working space. No one checked on the progress of the device during these several days and the most important consequence of finishing it was its installation in the control circuits of Elinor Maccoby's pigeon experiment.

I began two experiments as soon as I had straightened out the cabinets, swept the floor, and built the random-ratio programmer. One was variable-interval baseline with a time out between reinforcement and the performance that preceded. I don't remember now why I did this experiment except perhaps it was the only one I could think of. Fortunately, no one asked me. I was surprised that the delay between the performance and the operation of the food magazine did not decrease the frequency of pecking, so I continued to extend the delay period. No one noticed this experiment for some time. Skinner suggested the second experiment. He thought we should do something with "ratios" and we talked about how number of pecks could control the bird's behavior in a ratio schedule and I suggested that we reinforce for a long time on FR 50 and see whether we could see the evidence of the reinforcement after fifty performances, when reinforcement was discontinued. Skinner suggested a random alternation between a small and a large fixed-ratio schedule (two-valued ratio) so that the control by the smaller ratio would show up in the effect

on the large ratio on a continuing basis. The idea of a stable state experiment ended the discussion and began the experiment.

Thereafter our discussion about experiments occurred at "rounds," usually the first thing each morning when we toured the laboratory to look at the harvest from the day and night before. This was when we discovered the apparatus failures, particularly in cumulative recorders, which were so frequent and discouraging during the early days of the pigeon lab. Failures of programming and recording sometimes set an experiment back the days or even weeks that were necessary to recover the baseline. On these occasions Skinner always commented on what caused the failure and we discussed changes that would reduce the likelihood of failure in the future. Although both of us felt keen disappointment in the delay in the experiment, our remarks always concerned possible remedial action rather than the current failure of the experiment (or perhaps the experimenter). Rounds took thirty minutes to an hour, depending on the press of other activities, and it was a lively activity with much rolling and unrolling of cumulative records, comment on what had happened, ooo's and aaah's about a new degree of orderliness and planning of the next procedure. Conversations did not include references to who had pulled the switch, first mentioned the idea for the experiment, built the apparatus, or predicted the outcome of the experiment. It took almost a year before I stopped predicting. The pigeon really did know best what it was he was likely to do and the conditions under which he would do it. Free of Skinner's praise, I was also free of his censure, real or imagined. Yet I still had the advantage of an inspiring model I could observe, whose behavior prompted me to greater accomplishments. I remember how easy it was for me to talk with Skinner about experiments and psychology in general. I sometimes wondered how it was that this young man could face the feeling that almost anything he could do Skinner could do better. I think the reason I could contribute my portion without uneasiness was that I was never evaluated, rewarded or punished; nor was my behavior ever measured against his. I found Skinner's repertoire an ever-present source of prompts and supports which I could use whenever I was

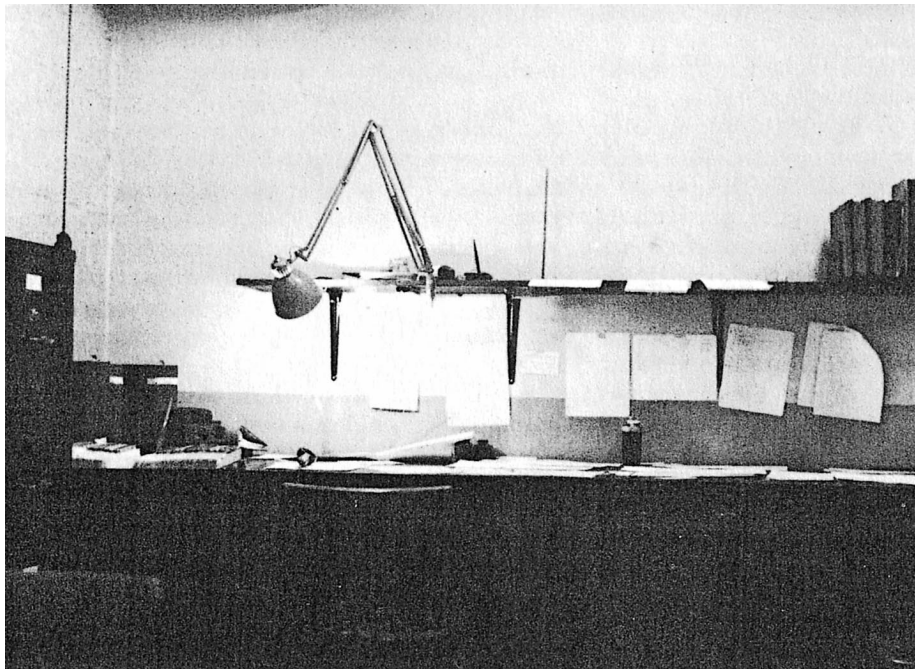


Fig. 2. The room where the graphs were pasted up and where schedules of reinforcement were written.

able to. It was a very fortunate young man from Columbia who had an opportunity to carry out his work with so much intellectual and practical support and with such exciting chances to "brainstorm." Nor was it a small measure of support to be able to watch B. F. Skinner in the laboratory designing new instruments, or to be able to turn over a problem to him.

But I give the reader the wrong impression if I suggest that there was no reinforcement for the results of experimentation other than the actual behavior generated in the birds. There were many personal, natural consequences of completing a successful experiment. A successful experiment led to conversations about the data, the new devices we could build, the new experiments that had to be started and the new ways we could organize our past experience from the laboratory.

When we discovered a new degree of orderliness or an unexpected but rewarding result on morning rounds, there was always much excitement and talk about where the experiment might go next and how to manage the equipment for the next experiment that was burning to be done because of the new result. When new discoveries accumulat-

ed too fast to be digested during morning rounds, there were planning sessions which were always great fun and very exciting. It was during these sessions that I learned the value of large sheets of paper which we used to aid our thought and to chart our progress. Every experimental result appeared as an entry someplace on paper about ten square feet in size. The theoretical structures and programmatic aspects of our work appeared as the spatial arrangement of the headings. Later, these headings were to appear as chapter and subchapter titles in *Schedules of Reinforcement*. Each entry prompted rearrangements of the theoretical pattern and suggested new experiments and programs which in turn prompted further rearrangements of the data. The interactions between these theoretical exercises and changes in ongoing experiments in the laboratory were continuous and constituted an important reinforcer. Although almost all of the entries on the large sheet of paper were in Skinner's hand, I took part significantly by providing facts and prompts, and by reacting to the patterns which emerged. Mostly we arranged and rearranged the findings and procedures we had discovered and worked with, and reacted to the new experi-

mental procedures that were suggested by the arrangements. Skinner did most of the talking, just as he did most of the writing, but even when I was silent, I was always intensely involved because he generally spoke for both of us. It was sometimes like playing a well-tuned organ that could play itself if the right key were pressed and a properly reactive listener were present. We came to share such an extensive repertoire that not everything had to be said by each person. When one person spoke, the other frequently could have said the same thing a few minutes later, or he might have been so close to saying it that a small amount of supplementary stimulation was enough to produce the same performance.

Our interaction as speakers and listeners was an apt illustration of the verbal process described in *Verbal Behavior* where Skinner wrote, in the chapter on supplementary stimulation, about strengthening the behavior of the listener. To a degree we were in the same position as speakers and listeners as the proverbial prisoners who told jokes by code numbers, indicating stories that they already knew. "When no one laughed when the taps on the pipe indicated Joke Number Ten, it was explained to a visitor that this prisoner didn't tell jokes very well."

One of the unspoken rules of these thinking, planning and theory sessions was to avoid criticism or contradiction. The performances which occurred were delicate and of such high frequency that criticism or contradiction produced a large and sudden change. I learned that there were natural consequences of unproductive or incorrect suggestions or formulations which shaped them and altered their frequency. Any thought was fair game and the worst that could result from an error, an inept or an inappropriate suggestion was that it would be ignored or have no consequences in prompting or aiding other activities. If one or the other of us had strong behavior which was not shared, a record was made on the work sheets, apparatus was built, or an experiment was started, but there was no requirement for both to participate or speak about it. In some cases an unshared line of work disappeared because the performances it led to were not useful. In other cases it persisted successfully.

I don't remember any experiment being

called "great" or "bad" or anyone being given credit for doing something especially useful or valuable. Some experiments led to further planning, new apparatus, exciting conversations, new theoretical arrangements of data and procedures or a rush to tell everyone about them, while others enabled less behavior of this kind. I don't know whether Skinner was conscious of the lack of personal praise in interpersonal relations in the laboratory. I certainly was not. My behavior was generated by the natural reinforcement of the laboratory activity. But some of the graduate students found the absence of personal support difficult.

Recently a distinguished psychologist, who had come to Harvard when he was a student to study under Skinner in the pigeon lab, reminded me of an incident which illustrated the personal styles around the laboratory then. After completing the professional seminar, the main classroom experience in the Harvard curriculum, he appeared before Skinner saying that he was ready to do research in the pigeon laboratory. He asked what he should start on. The conversation was awkward; the student did not receive the kind of support and encouragement that he expected, especially since he had come to Harvard for the single purpose of working under Skinner. Finally, in the heat of frustration, he complained, "Aren't I even going to get a pigeon box?" This remark galvanized Skinner who dashed out of his office into the pigeon laboratory around the corner shouting, "Charlie, he needs a pigeon box," and left. I dutifully took one of the unused Sears and Roebuck ice chests we used as the shells for pigeon experimental spaces, handed it to him, and left. The student was then left with the problem of assembling all of the components and constructing the equipment he needed. Although neither Skinner nor I remembered the incident, the anger and disappointment could be detected after all these years. Yet he went on to complete an experiment which was an original departure from the main experimental program of the pigeon laboratory and which still remains in the literature as a base for much research and thinking. I don't know whether this particular student would have gone on to do the same valuable work had Skinner supported his ideas personally, or had I given him equip-



Fig. 3. The members of the pigeon staff meeting posed for a picture toward the end of one of the meetings. Left to right are B. F. Skinner, Clair Marshall, W. H. Morse, R. J. Herrnstein, Tom Lohr, Nate Azrin, and James Anliker. Murray Sidman was visiting. Others attending frequently were Peter Dews, Ogden Lindsley, and Michael Harrison.

ment and supervised his day to day work in an experiment related to ours. But I think many others would have become pale imitations of Skinner and Ferster rather than the original, imaginative, aggressive scientists they did become.

The pigeon staff meeting where we reviewed current experiments with graduate students and others was one of the traditions of the laboratory. We met, usually weekly, in the seminar room, reviewing and talking about one or two birds. Ogden Lindsley introduced the symbol of the pigeon feather at this time when he made up a sign with a pigeon feather that was hung on the bulletin board on days that there were meetings. Later, he sent a white feather to the charter subscribers of *JEAB*. The seminar presentation consisted of either Skinner or me going through the cumulative records, a day at a time and a bird at a time, reacting to the small details of the results. The substance of the meetings was a very detailed examination of the results, even if some participants had to learn to read cumulative records upside down. There were frequent interruptions with questions, suggestions, or comments,

and usually a prolonged discussion at the end. When the prolonged discussion occurred before all of the data in the experiment had been covered, we continued with the same bird the next time. Later, when students and others had experiments under way, they brought in their data in a similar way. The presentations of the pigeon staff meetings were seldom a summarized formal report of what had happened in the experiment, but rather an informal scanning of the raw data. I think the feeling of participating in the formulation and identification of the results contributed to strong interest in the meetings.

Most of the research for schedules of reinforcement was completed by 1953, when we began to plan a written report. As more of my time shifted to organizing the data and writing, the laboratory was turned over to Morse, Herrnstein, and others at Harvard. By the end of 1954, Skinner and I were writing full time. The first problem we faced was how to present the large amount of data we had collected, not only from long experimental sessions and protracted experiments, but also from a large number of separate experi-

ments. By the end of our research there were about a dozen separate experiments in progress. The problem was to compromise between the need to report enough detail of our descriptive experiments and the need to reduce the bulk of the thousands of feet of cumulative curves. Three inventions—the collapsed record, a razor blade, and a standard cardboard stock thirty inches long—got the final report under way. Collapsing the record by cutting out blank paper along the time axis allowed us to present as much as fifty to seventy-five thousand pecks in a single figure; the razor blade made it possible to cut the records swiftly and effortlessly; and the card stock permitted a storage system that was easily handled. Skinner usually pasted records on the cards while I cut excerpts from the folders. Decisions about what to excerpt were made quickly, usually without much discussion because we were both so familiar with the records. Skinner took justifiable pride in his skill and speed with a razor blade. The ultimate test was to cut on several layers of paper, piercing an exact number of layers. The figures were pasted up, experiment by experiment, and the categories under which the figures were filed turned out to be the chapter and section headings of the book. On our best days we could do thirty figures, but this was a grueling pace which could not be kept up. Once the figures were completed, the writing turned out to be a relatively routine job of describing the main features of a record and indicating procedures. It became clear very early in our writing that we could not discuss the experiments theoretically or spell out the implications for the casual reader.

We worked slowly at first, but the need to finish before my scheduled departure in June 1955 led us to organize our environment and to develop several ways of self-management. All our work was done in a room dedicated to writing and not used at other times. Interruptions were the first problem, which we handled by a decision not to take phone calls. When visitors appeared at the door, we routinely stepped in the corridor to speak with them briefly. The frequency of interruptions became very low and the writing room came to control our behavior. Usually we began before nine and stopped by lunch time. There was frequently a temptation to continue in

the afternoon when we were working especially well or when the data were especially interesting, but our recently acquired data on fixed-ratio performances convinced us to seek a work schedule that kept our performance at maximum frequency for the period we were actually writing. The procedure worked very well. There were no warm-up or inactive periods in the writing room. Naturally we did not write elsewhere nor did we converse about outside matters nor do anything but work on schedules of reinforcement so long as we were in the writing room. At times the pace of the writing was so intense, and rewarding, that we began to control our outside activities in the fear that they might compete with or decrease the frequency of writing and graph-making. Bridge, chess and late social evenings were out.

The professional record speaks for the “before and after” of the pigeon laboratory. There were personal results too, however. B. F. Skinner has already written his feelings about our collaborative activities. For my part, besides the satisfaction of a very rewarding association, I remember most of all how I came away from the pigeon lab with a firmly developed attitude toward discovery and unknown things.

There is a fear of the unknown in research just as there is a fear of dealing with new people. We approach a new problem or a new person with a repertoire that comes from our past experience. When we are successful, the new person or problem differentially reinforces our existing repertoire, and we acquire a new means of dealing with a new environment. Unfortunately, the old repertoire often continues without significant influence of the new contingencies. Such a repertoire is called compulsive or neurotic by clinicians. The analogue, in research, is the experimenter who is controlled primarily by the social and professional consequences—his colleagues’ verbal behavior—and to a lesser degree by the behavior he produces and measures in his experiment. I don’t think we were ever worried in the pigeon lab that we would have nothing to show for our time or that an experiment would waste time and money. The pigeon lab was a place where an unknown problem became an occasion which led to discovery and accomplishment rather than a cause for worry. The more a new situation could be seen

as very different from our current experience, the more it signalled an experiment that would bring results which we valued. Perhaps my experience in the pigeon lab with B. F. Skinner prompted me to write in 1958: "A potential reinforcing environment exists for every individual, however, if he will only emit the required performances on the proper occasions. One has merely to paint the picture, write the symphony, produce the machine,

tell the funny story, give affection artfully (manipulate the environment and observe the behavior of the animal) and the world will respond in kind with prestige, money, social response, love (and recognition for scientific achievement)."<sup>1</sup>

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<sup>1</sup> Ferster, C. B. (1958, December). Reinforcement and punishment in the control of human behavior by social agencies. *Psychiatric Research Reports*, 101-118.